Anonymous Referee #2

We would like to thank referee 1 for his/her constructive and detailed review.

This paper examines the physical processes responsible for the AMOC response to reduced solar radiation and assesses the importance of chemistry-climate in modulating this response. By comparing two sets of climate model experiments, with and without interactive chemistry, the study demonstrates that climate models which do not consider stratospheric -namely ozone -chemistry may overestimate the sensitivity of the AMOC to solar forcing since the “top-down” influence (stratospheric influence on tropospheric circulation) is underestimated.

In my opinion, this work constitutes a very nice contribution for the broad climate research readership as it demonstrates, using the specific example of the AMOC, the prominent and complex connections between the different components which drive climate variability (going from the stratosphere chemistry to the ocean circulation). The work is well-framed in the current literature. I found the paper mostly clear, well written and scientifically sound. I think however that some improvements and clarifications could be made before publication. Please find my main comments/suggestions below:

Main comments/questions:

1. It has already long been recognized that atmospheric chemistry interacts with dynamics and that its consideration in climate models is crucial to adequately simulate climate variability (e.g. influence of the ozone hole recovery on the SAM trends in CMIP3 simulations by Son et al. (2008)). As a consequence, historical and projection climate simulations in CMIP5 for models without interactive chemistry were designed by prescribing chemical fields that consider long-term trends (Cionni et al., 2011). For CMIP6, the ozone prescription fields should be even further improved. So I would say that the current question regarding chemistry-climate interactions is: do we really need interactive chemistry? or can it just be prescribed? The other question is then how to prescribe it in the most accurate way (see e.g. Nowack et al. (2015)).

In my opinion, given the frame, the results and the conclusion of the present study, I think that the introductory part of the paper should –at least partly –review the recent advances regarding chemistry climate interaction. A lot has been done already and should not be ignored.

Thank you, we will add a discussion of previous studies in chemistry-climate interactions in the revised manuscript.

2. In the light of my previous comment, I would suggest the authors to explain more thoroughly how the combined UV+ozone effects modulate the heating rates in the stratosphere which is the starting point of the stratospheric mechanism discussed in the paper. The thermal modulation of the stratosphere through UV variations comes from two main effects: (1) direct shortwave heating through incoming UV absorption by ozone (λ~200-300 nm), (2) ozone change (λ<242 nm) which also affect shortwave heating rates. Both effects count significantly. Basically, and if I understood correctly, their NOCHEM experiment account for effect (1) only while CHEM account

for effects (1) + (2). I think such clarifications are easy to make and necessary since they help understanding the basic difference between the two experimental configurations (at least regarding stratospheric ozone which is the major solar effect). In the present version of the paper too few information are given on UV-ozone-temperature interactions and their implication on experimental setting (e.g. P2L32-P3L1, P5L12-14).

We will improve the description of the UV+ozone effect in the revised manuscript.

3/-A very recent study by Chiodo and Polvani (2016) has just been released in *Journal of Climate* and deal with –somewhat -similar problematics. They performed simulations that also present some similarities with those perform in the present work. While both studies have their own relevance and focus on different aspects, they also nicely complement each other. The authors may consider comparing results of both studies: are they consistent?

Thank you. We have added a comparison to the results of Chiodo and Polvani to the discussion section of the revised manuscript:

"Recently, Chiodo and Polvani (2016) assessed the role of the interactive chemistry on the temperature and precipitation response to increasing SSI. They identified a reduced sensitivity with interactive chemistry due to the effect of the ozone increase on the short-wave radiation balance. Our results for a SSI reduction indicate a slightly larger temperature sensitivity with interactive chemistry owing to the effect of the stratospheric water vapour and ozone changes on the long-wave radiation balance. These differences may be attributed to model differences or differences in the response of the climate system to increasing and decreasing solar forcing. A possible effect of the differences in the atmospheric response on the AMOC is not discussed by Chiodo and Polvani (2016)."

4/-In light again of my first comment, there is currently a debate about the need of having interactive chemistry in climate model or if it is sufficient to prescribe chemistry. The concern is real given the heavy computational costs that interactive chemistry requires. This question could have been addressed here by using the chemistry outputs of the CHEM experiments as a chemistry forcing for a say "prescribed-CHEM" experiment with solar-induced ozone changes. Both effects (1)+(2) (see comment 2/) could thus have been considered without including interactive chemistry. Did the authors perform such experiments? If they have (and only if they have), it would be relevant to mention their conclusions in the paper.

We agree that this is a highly relevant question. Unfortunately, we did not perform these simulations.

Specific comments:

+ P1L6-10: "*In simulations with chemistry-climate interactions a second, dynamical effect on the AMOC is identified which counteracts the thermal effect. This dynamical mechanism is driven by the stratospheric cooling in response to the reduced solar forcing, which is strongest in the tropics and leads to a weakening of the Northern polar vortex. In simulations with interactive chemistry, these stratospheric changes are strongly amplified by the reduction of stratospheric ozone.*" The point made in these three sentences seems confusing. The first two sentences seem to suggest that the stratospheric cooling is found only in the chemistry-climate simulations while it is in fact found in both but amplified when ozone reduction feedback is included (as suggested by the third sentence) in addition to the direct radiative heating reduction. This may benefit of being clarified.

The abstract will be rewritten.

+ P2L12-13: "*The variability of the overturning circulation is furthermore influenced by external forcings (Otterå et al., 2010). Volcanic eruptions have been found to intensify the AMOC on decadal time scales (Otterå et al., 2010; Mignot et al., 2011).*" Since the study particularly investigates the mechanisms, I would suggest here to specify through which mechanisms volcanic eruptions influence AMOC (i.e. direct radiative cooling effect + tendency to induce positive NAO).

Rewritten to "Volcanic eruptions have been found to intensify the AMOC on decadal time scales (Otterå et al., 2010; Mignot et al., 2011), through a reduction of the SSTs and a shift of the NAO towards a positive phase."

+ P2L21: change "*trough*" to "through"

Done.

+ P2L34-P3L1: "*This response is modulated by chemistry-climate interactions. In particular, stratospheric ozone reacts to the UV changes and amplifies the stratospheric temperature change (Baldwin and Dunkerton, 2005)*". I think that further explanations on the UV-ozone-temperature interactions may be needed given that they are the source of the difference found between the CHEM and NOCHEM versions of a same experimental scenario. Furthermore, the reference to Baldwin and Dunkerton (2005) might not be the best suited for this purpose. The authors could rather refer to the work of J. Haigh in the 1990s (Haigh, 1994 ; 1996). The authors could also refer to section 3.5 of the CCMVal report (and reference therein) which can be found at the following address http://www.sparc-climate.org/publications/sparc-reports/sparc-report-no5/. This chapter particularly details the implication that prescribing constant ozone (as in the NOCHEM experiments of the present study) has on shortwave heating rates associated with changes in the UV.

Thank you, we will consider this comment and improve the discussion of the UV+ozone+temperature interactions in the revised manuscript.

+ Section "*2.1 The model*": What about energetic particle effect? SOCOL-MPIOM has parameterizations that allow taking into account GCR and EPP effects (which are linked to solar activity variations) and are suggested to also have an impact on the Northern Hemisphere surface climate (e.g. Rozanov et al. (2012)) through the "top-down" mechanism and thus may also affect the AMOC.

SOCOL-MPIOM includes an EEP and GCR parametrization, but we concentrate on the effects of solar irradiance keeping the same EEP and GCR because they should not be changed in SRM case. Therefore, we think that these processes are not substantially relevant for our study and do not need to be mentioned in the model description.

+ P5L12-20: Here the authors may consider discussing their results in comparison with Chiodo and Polvani (2016).

We have included a comparison with the results of Chiodo and Polvani (2016) in the discussion section of the revised manuscript *see above).

+ P6L4-5: The sea-ice extension and the associated differences between S2-CHEM and S2-NOCHEM experiments are hard to see on Fig 2 which is already quite busy.

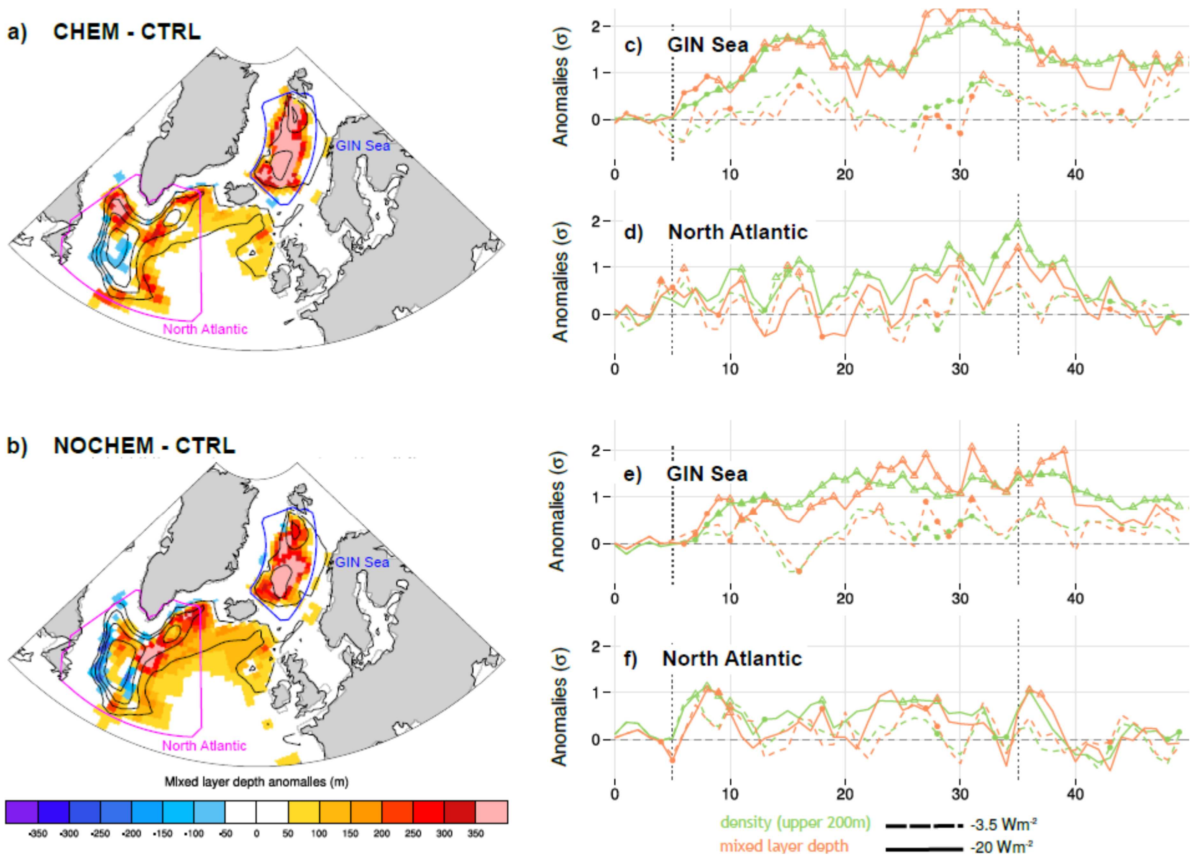
We agree, therefore the sea ice is shown again on Fig. 3. However, given the large number of figures we prefer not to add additional figures to the manuscript.

+ P6L13-14: "*Additionally, a significant reduction of the precipitation is found in the North Atlantic, which further increases the salinity*." Please indicate that this is not shown (in brackets).

Done.

+ P6-7: "*3.1 The thermal effect of SRR on the AMOC*": This part contains very interesting material and is very informative. However, I found quite hard to follow the text and figures together. While this is largely due to the fact that I am not used to examine ocean processes, I believe that some improvements could still me made. In particular, one of the key points relies on the differences, between the CHEM and NOCHEM configurations, of the timing of the anomalies development leading to differences in the AMOC response. In this regard, I think that, in addition to spatial patterns (Fig. 3), showing time series (similar to Fig. 1) of the key variables in the key regions may help understanding the timing issue.

We show time series of the mixed layer depth and upper ocean density for the two convective regions (GIN Sea, i.e., Nordic sea, and North Atlantic) below. However, we prefer not to include these time series in the revised manuscript. The time series are dominated by large variability and it is very hard to identify clear differences between S2_CHEM and S2_NOCHEM from these figures. The average over two 15 years periods removes a lot of the year to year variability and helps to identify the main differences between the two ensemble experiments.



R 5: Anomaly maps (averaged over the solar minimum) and time series of the mixed layer depth and upper ocean density for the two convective regions (GIN Sea, i.e., Nordic sea, and North Atlantic). S2_CHEM is shown in panels a), c) and d). The results of S2_NOCHEM is shown in b), e), and f). Blue and magenta boxed in a) and b) denote the areas for the time series. The time series of the mixed layer depth and upper ocean density anomalies are normalized using the mean value and standard deviation of the corresponding control experiment.

+ P7L19-20: "*For S2_CHEM, a pronounced weakening of both polar vortices is found*." Please give the reference to Figs. 4d,e,f in the text and replace "*both polar vortices*" by "NH and SH polar vortices" for clarity concerns.

Done.

+ P7L25: Is it annual anomalies or only winter (NDJFM) anomalies which are shown in Figs 4 and S3? Please clarify.

Fig. 4 and S3 show the annual mean anomalies. This is stated in the caption of Fig. 4 in the revised manuscript.

+ P7L26-27: Again for clarity, one sentence to explain what a SSW is may be useful here.

We have added a short explanation on SSWs:

"The weakening of the NH polar vortex is closely related to the occurrence of sudden stratospheric warming (SSW) events (Fig. 5). SSWs are stratospheric extreme events, in which the westerly flow during winter time is reversed and a strong warming in the polar stratosphere can be observed. SSW events in the NH are associated with a 'break down' of the polar vortex."

+ P8L3-4: "Overall, the downward coupling of wind speed anomalies does not differ substantially between the CHEM and NOCHEM control experiments." Although it is written that the statement concerns "anomalies", I believe that this sentence might be misleading since it seems to suggest that the CHEM and NOCHEM downward influence of the stratosphere on the troposphere are the same. We thus may wonder why we should expect a difference in the AO strength (described in paragraph which follows, P8L5-14). Please make this point clearer (as it is a key point of this paper).

We do not find large differences between the two control experiments, suggesting that the interactive chemistry has no large effect on the dynamics and the variability, when all external forcings are kept constant. Consequently, the influence of the stratosphere on the tropospheric AO is comparable with and without interactive chemistry. This has also been found in earlier studies with SOCOL-MPIOM (compare Muthers et al. 2014.).

However, this does not mean, that no differences is found when a changing external forcing is applied. In fact, we show in our results, that the interactive chemistry leads to a strong differences in the stratospheric temperature change to the reduced solar forcing, which causes a stronger weakening of the Northern polar vortex, which in turn leads to a clear difference in the response of the AO. This response is not related to differences in the stratosphere-troposphere coupling between both experiments, but to a differences in the stratospheric response.

References:

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